mis, oblongis, pilosis: pedicellis glabriusculis; Gluma inferiore flosculis triplo breviori 1-, superiore eosdem aequante 7-nervi; Hermaphrodito elliptico, laevi neutrum aequante.

Panicum laxiflorum Spreng. in Mém. de St. Pétersb. II. p. 291.

Panicum heterophyllum Mühlenb. teste N. ab Es.

V. spp. Am. bor. (TRATTINICK).

Culmus tenuis, adscendens, basi ramosus. Folia, quorum plura basi plerumque confertissima, lineari-lanceolata, cum vaginis pubescentia, pollicaria, lineas 2 lata: superiora angustiora, dissita. Panicula ovata, axis radiisque glabris. Flosculus neuter bivalvis. Hermaphroditus albescens.

Label accompanying type specimen: "Pan. heterophyllum Muhl. (Test. Nees) an Pluckn. Tab. 92 f. 8? ex herb Enslini, spmna Am. bor. Trattinick."

Specimen = *P. columbianum* Scribn. 1897. Div. Agros. Bull. 7:78.

In recent works this name has been applied to a species of the *lanuginosum* group having rather stiff foliage and the leaf blades hirsute on both surfaces. The true *P. unciphyllum* is easily recognized by the short crisp pubescence and the very short ligule, characters not mentioned in the original description.—A. S. HITCHCOCK, *U. S. Dept. Agric., Washington, D. C.*

SPOROGENESIS IN PALLAVICINIA.

THE August number of the BOTANICAL GAZETTE contains a paper by Mr. A. C. MOORE on *Sporogenesis in Pallavicinia.* I regret again to ask for space on this matter, but Mr. MOORE has so completely (though of course inadvertently) misrepresented my own position with regard to the nature and the significance of the quadripolar spindle in the Jungermannieae, and further, the grounds on which he founds his criticism appear to me to be so open to objection, that I venture to ask for an opportunity of replying to his strictures.

Firstly, then, as to the significance attached to the quadripolar spindle in 1894–5.

From Mr. MOORE'S account it would seem that I regarded, as the most essential feature of its importance, the simultaneous distribution of the chromosomes of the dividing nucleus of the mother-cell to the four spores that are finally produced.

I certainly believed that in *Pallavicinia decipiens* such a distribution occurred, and that it resulted from the suppression of the period of rest normally intervening between the first and second maiotic divisions. In this I may be right, or further investigations may show that, in the species in question, I missed the binucleate stage, But this is really not the essential matter at all. The result of my work published in 1895 went to show that in most forms there are two consecutive mitoses, the second, following more or less rapidly on the first, and I believed that in *P. decipiens* the brief interval might be so shortened as to have become practically obliterated.

But the circumstance that quadripolar spindles were shown by me to be plainly visible in properly fixed material of forms in which no such extreme telescoping of the normal sequence of events takes place, clearly proves that, whatever the significance of the quadripolar spindle may be, it certainly is not *essentially* related to a simultaneous distribution of the chromosomes amongst four daughter nuclei, and I never thought it was.

What I believed in 1895 (and I have seen no reason to materially alter my view), was expressed as follows: "The quadripolar spindle, then, is only a special case of ordinary karyokinetic phenomena; instead of two relatively large masses of protoplasm there are four distinct aggregations, one in every lobe, each exercising an independent strain, and the direction of the strains may continue separate to the very end of the process or not, according to the form and special circumstances of the cell."[1] I may perhaps add, that the principal importance of the phenomenon, in my view, lay in its bearing on the permanence of the centrosomes, at that time a widely accepted doctrine.

In the second place, Mr. MOORE seems to think that his observations on *P. Lyellii* vitiate the conclusions based on a study of *P. decipiens*. I venture to think they do nothing of the sort. It is clear that the two species differ in the form of their spore mother-cells to a marked degree, and also that this difference is exactly of a nature to account for the unequal persistence of the peculiarities of the spindle in the two cases. For the lobing of the spore mother-cell is so much less in *P. Lyellii* than in the other species, that it would be a matter for surprise if the quadripolar character of its spindle were so long retained.

I confess, however, that I should have expected centrospheres to be present at the stages represented in *pl. III, figs. 1–3* of Mr. MOORE'S paper. They are so obviously demonstrable in *Aneura pinguis* and in *Fossombronia pusilla*, the spore mother-cells of which resemble in their lobing those of Mr. MOORE'S plant.

One feels a little difficulty in repressing a suspicion as to the successful fixation of his material, a suspicion not dispelled by the further contemplation of *figs. 12* and *13*. They so faithfully depict preparations I have

[1] Annals of Botany 9:508.

myself very often obtained when the fixation had been imperfect. It is, of course, easy in these plants to secure admirable preparations of the stages preceding and following on the maiotic divisions, but I am sure Mr. MOORE will agree with me as to the great difficulty encountered in successfully fixing the cell contents at this critical period. Personally, I have not found chromacetic acid (the fixative used by him) very successful, but obtained far better results with Flemming's solution and, if due precautions are taken, with acetic alcohol. The latter, in particular, has yielded results of especial excellence, owing partly, no doubt, to the relative rapidity with which it traverses the somewhat impervious cell wall.—J. B. FARMER, *Royal College of Science, London.*

REPLY.

PROFESSOR FARMER acknowledges that in 1894 he believed in the simultaneous distribution of the chromosomes to the four spores in *Pallavicinia decipiens.* His description stands as the only account of a process without parallel in the plant kingdom, and he must have realized its exceptional nature. The account became all the more remarkable when Professor FARMER'S own studies on a number of liverworts, published in the following year, showed two successive mitoses in the spore mother-cells as in other groups of plants. He acknowledges now that he may have missed the binucleate stage. This is precisely what I believe he did, but since I have not investigated *P. decipiens* I cannot assert that he did so. Now he states that this simultaneous distribution is really not the essential matter at all. Apparently the essential matter to him is his observation that several liverworts conform to the normal sequence of nuclear division during sporogenesis. Yet these conclusions, bearing as they do on *Pallavicinia decipiens,* served to emphasize the peculiarities of that account, and I feel confident that most, if not all, cytologists would pick out the description of a simultaneous distribution of chromosomes as the most essential feature of his paper of 1894.

I venture to think that botanists are not so much interested in the explanations which Professor FARMER may make of what he did or did not believe in 1894 and 1895 relative to the quadripolar spindle (which opinions they can form for themselves), as in the facts of sporogenesis in the liverworts. My study of *Pallavicinia Lyellii* is plainly a challenge of his account of *P. decipiens,* and together with Professor DAVIS'S work on Pellia, leads us to believe that the "quadripolar spindle" in all liverworts is a phenomenon of prophase followed by spindles of two successive mitoses, in essential agreement with the events of sporogenesis in other plants.